XXVIII. *A Supplement to Monf.* Pingré's *Memoir on the Parallax of the* Sun : *In a Letter from him to the Royal Society,* *Tranflated by* M. Maty, *M. D. F. R. S.*

Gentlemen,

Read May 17, 1764. I Had the honour to fend you my memoir on the Parallax of the Sun as deduced from the obfervations of Venus; give me leave to add to it fome remarks which I have fince made, and communicated to our academy. The learned Societies of Europe, amongft which yours holds a moft diftinguifhed rank, will be the judges of the fuccefs of our expeditions. A definitive decifion will probably not be formed till after the obfervation of the tranfit of 1769; in expectation of further accounts, which may tend towards this decifion, there is at leaft one which I now fubmit to your examination.

Supplement to my Memoir on the Parallax of the Sun.

I impatiently expected the firft volume of the Philofophical Tranfactions of the year 1761, where I hoped to find fome obfervations that might determine which of the obfervations, *viz.* that of Meff. Mafon and Dixon made at the Cape of Good-Hope, or that which was made at the ifland Rodriguez by Mr. Thuillier and myfelf, deferved the preference. The firft reduces the Sun's Parallax to $8''\frac{1}{2}$ at moft,

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whereas the latter increafes it to near $10''\frac{1}{2}$: the difference is too confiderable not to deferve an enquiry into its caufes. The expected volume is at laft come to hand; and my firft care has been to examine with the moft fcrupulous attention the obfervation made at the Cape. I owe this teftimony to truth, that this obfervation, as well as all the others of Meff. Mafon and Dixon, appears to me to have been made with great judgment. An exception might however be made as to the extenfive defcription of an appulfe of σ Sagittarii to the Moon's fouthern limb, which is found page 389. This certainly was more than an appulfe, and the ftar was really immerged at the Cape.

In this fame volume, I found two obfervations, which would be decifive, if time and other circumftances had permitted them to be made with fufficient accuracy. I have very carefully calculated them both. Mr. Mafkeline obferved at the ifland of St. Helena, fituated at 15^d $55'$ South latitude, and according to Dr. Halley at $33'$ $17''$ of time Weft of the Obfervatory at Paris; but this determination of the longitude does not feem fufficiently exact. I have compared many obfervations of Jupiter's Satellite's immerfions and emerfions made at the ifland of St. Helena by Mr. Mafkeline with the correfponding ones made at Paris at the Marine Obfervatory by Mr. Meffier, and have only found $31'$ $56''$ for the difference of longitude between the two places; and as the Marine Obfervatory is $2''$ Eaft of the Royal Obfervatory, I think I may conclude that the place where Mr. Mafkeline obferved is only at $31'$ $54''$ Weft of the Royal Obfervatory.

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The clouds greatly obftructed Mr. Mafkeline's obfervations; he could make but one that was ufeful. At $7^h 31' 07''$ in the morning, apparent time, the bodies both of the Sun and of Venus being perfectly well defined, the diftance of the neareft limbs was, by means of an object-glafs Micrometer adapted to a reflecting telefcope according to Mr. Dollond's invention, found to be $1'44''\frac{3}{4}$. I have computed that by allowing $10''$ for the horizontal parallax of the Sun, from that phafis to the internal contact of the limbs, there muft have paffed $34' 52''$ at St. Helena; I have even affured myfelf, by fome other calculations, that, by an alteration of one or two feconds in the parallax, and of feveral minutes of time in the longitude of St. Helena, the interval which I have juft now determined would fuffer an increafe or diminution but of very few feconds. The internal contact of the limbs muft therefore have happened at St. Helena at $8^h 05' 59''$ in the morning. One can't fuppofe it to have happened later, becaufe $17'$ after, or at about $8^h 23'$, the fkies having cleared again, nothing more could be feen, and the external contact was over. This obfervation being compared with that of Tobolfk, would give $11''$ for the horizontal parallax, which is a little too much. Mr. Mafkeline obferves, that tho' Venus's limb and the Sun's appeared as defined as could be defired, yet when the artificial internal contact of Venus's limb with the Sun's was made by means of the object-glafs micrometer, Venus's limb dilated and contracted itfelf alternately, getting and lofing a fmall fpace within the Sun's limb. He adds, that he endeavoured to take it in the middle of this vibration, but dares not affirm that he exactly

actly did so. Let the diftance determined by Mr. Mafkeline be only diminifhed by 2″, and his obfervation will perfectly agree with mine; but in order to make it tally with that of the Cape, it would be neceffary to diminifh this diftance by 10 or 11″, and it is not very· likely that Mr. Mafkeline fhould have committed fuch an error. His obfervation might likewife be brought to coincide with mine, by making a diminution of 40 or 45″ in the Weftern longitude of St. Helena, as I juft now fettled it; whereas it would be neceffary to take feveral minutes from that longitude in order to make the obfervation agree with that of the Cape, which does not feem poffible. Hence, though I don't take Mr. Mafkeline's obfervation to be in itfelf abfolutely decifive, yet I am perfwaded that it adds great weight to the exactnefs of mine.

Fort St. George at Madrafs is, according to Mr. Hirft, $13^d 8'$ North latitude and $3' 4''$ of time Eaftward of Pondicherry, and confequently at leaft $5^h 12' 54''$ Eaftward of our Obfervatory's meridian. Mr. Hirft's obfervation is related in the Philofophical Tranfactions; the interval obferved by him between the two internal contacts was $5^h 51' 43''$, greater by $2' 49''\frac{1}{4}$ than that which was obferved at Tobolfk. This would give $9'' 56$ for the Sun's horizontal parallax, a quantity which is about a medium between the Cape obfervation and mine. The comparifon between the time of obferving the contacts, and that of the correfponding obfervations made in other places, gives conclufions fo vaftly different, that I dare not relate them here. Mr. Hirft gives a fufficient account of his inftruments, but he does not fay whether his

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pendulum or clock was one with weights. He says he regulated his clock by equal altitudes, and then by meridional paffages of Spica Virginis, and of the Sun; but why did he leave off the method of the equal altitudes which he made ufe of at firft, and in what manner did he obferve thefe altitudes to determine the paffage either of a fixed ftar or of the Sun over the meridian? It does not appear that he had a quadrant or tranfit inftrument. I am forry that this uncertainty about the means employed by Mr. Hirft to determine the time of the phafes puts it out of my power to make ufe of an obfervation, which might otherwife have been extremely ufeful, had the aftronomer been equally well provided with inftruments as he appears to have knowledge and zeal. It is to be obferved that by increafing or diminifhing by 10″ the duration obferved at Madrafs, the queftion of the parallax will be decided conformably either to the obfervation of Rodriguez or that of the Cape.

In the fame volume of the Tranfactions, are fome obfervations of the fame tranfit made at Abo and at Hernofand; the total duration was obferved in both places; it may have been lengthened fomewhat beyond its limits; but thefe obfervations agree at leaft in this point with all the others that were made in the North, viz. that being compared with the Tobolfk obfervation, with regard to the duration of the tranfit, they give above 10″ for the horizontal parallax of the Sun.

I have likewife lately had the communication of Mr. Rumowfki's obfervation made at Selenginfk in Siberia. I fhall not expatiate upon the particulars here,

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suppofing that you Gentlemen have received it. The latitude of Selenginfk is $51^d6'6''$. I have fettled the longitude to be $6^h57'50''$ from the Paris meridian. Some immerfions of the firft and fecond fatellite of Jupiter have given me $6^h57\ 15''$, $6^h57'20''$, and $6^h58'31''$, by comparing the obfervations of Mr. Rumowfki with the tables corrected upon the obfervations made at Paris and at the Cape, and eftimating, as well as I was able, the different effects of the reflectors and telefcopes. The 15th of July, Mr. Le Monnier obferved the meridional tranfit of φ Sagittarii at $18^h25'38''\frac{2}{3}$ of his clock, being at $76°00'45''$ from the zenith. The preceding limb of the Moon paffed at $18^h38'28''\frac{1}{2}$, or at $11^h\ 01'40''$ apparent time; when the center paffed, the diftance of the upper limb was at $75^d52'00''$ from the zenith, and the lower at $76^d22'25''$; the threads, which are $5''$ thick, being entirely upon the Moon. σ Sagittarii had paffed at $18^h35'22''\frac{1}{2}$, the diftance from the zenith being $75^d22'20''$. I have obferved at Rodriguez the immerfion of σ Sagittarii at $14^h01'28''\frac{1}{2}$ apparent time, and Mr. Rumowfki obferved at Selenginfk the immerfion of φ at $11^h24'51''$ apparent time. Upon comparing all thefe things together, I find the longitude of Selenginfk to be $6^h57'21''\frac{1}{2}$ Eaft from Paris. But this goes upon the fuppofition that the error of the tables has been quite conftant during near 7 hours, which cannot be warranted. Laftly, the obfervation of the eclipfe of the Sun made at Selenginfk June 3, 1761, compared with the fame obfervation made at Tobolfk and at Cajanebourg, determines the longitude of Selenginfk $5^h16'41''\frac{1}{2}$ Eaft from Cajanebourg, and at $2^h34'30''$ Eaft from Tobolfk, confequently $6^h58'22''$

Eaft

Eaſt from Paris. Theſe are the reaſons which make me believe that the longitude of Selenginſk cannot be made leſs than $6^h 57' 50''$ from the meridian of our Royal Obſervatory. This being ſuppoſed, the obſervation of Mr. Rumowſki compared with mine would give $10'' 1$ for the horizontal parallax of the Sun in the month of June, and $10'' 26$ for that parallax when the Sun is at the mean diſtance. 'Tis true, the ſame obſervation, compared with that of Meſſ. Maſon and Dixon, would much reduce this parallax"; and therefore it can be of no uſe to decide the queſtion, unleſs we had ſome from Africa or from the adjacent ſeas.

I have one, which I have not dared yet to lay before our Academy ; it appears to me a downright phenomenon. As it was made at the iſle of France or Mauritius, it might pretend to the glory of deciding between Meſſ. Maſon and Dixon and me ; but it is ſuch a one as can only at moſt determine how far the difference of ſights or that of teleſcopes could extend or ſhorten the duration of the exit. I am acquainted with the obſerver, Mr. de Seligny, an officer in the ſervice of the Eaſt India company; and I think I can anſwer for his capacity, talents, zeal and accuracy. He had no inſtrument but an eight foot teleſcope, and an excellent clock: this he regulated by altitudes of the Sun taken the 5th and 6th of June with Hadley's quadrant. That method is not ſtrictly true, but our obſerver could do no better ; and beſides, whether the clock went too faſt by a few ſeconds more or leſs, the difference cannot at all affect the duration obſerved by Mr. de Seligny. His clock on the 5th of June about 3 o'clock advanced $18'' \frac{1}{2}$ upon the

the apparent; the 6th at the fame hour it got only 9″. The obferver was obftructed by the clouds till near the inftant of the exit of Venus. This is his obfervation of the exit; the times are thofe of his clock, from which 2′10″ are to be fubftracted to have the apparent time. At $0^h 28′ 38″$ the preceding limb of Venus appeared to touch exactly that of the Sun; at $0^h 22′ 00″$ one quarter of her diameter was got out; at $0^h 25′ 10″$ Mr. Seligny judged her to be half got out; at $0^h 28′ 06″$ the three quarters of her diameter were emerged, and $0^h 31′ 12″$ the exit was total and inftantaneous. Thefe obfervations follow one another very well, and they have been made by a man whom I know to be very fkilful; but how the exit of Venus could appear to him to have taken up but 12′34″ I muft leave to perfons fkilled in natural philofophy to account for. The obfervation of the firft contact, or of the internal contact of the limbs, would give you more than 12″ for the parallax; that of the laft would reduce it to eight; by a medium, the parallax would exceed 10″.

You will undoubtedly have obferved a pretty remarkable difference between my obfervations of Venus, as I had the honour to fend them to you from Lifbon March 6, 1762, and the fame obfervations as I have related them in my memoir. I imagined I had fent the correction to fome one of the members of your celebrated Society. When I made the reduction of thefe obfervations at Rodriguez, I found myfelf under difagreeable circumftances; and it was probably my uneafinefs at that time that occafioned a want of due attention, which I was the longer in 'finding out as I could not eafily fufpect

pect it. My clock went too flow that day at noon by 2′2″, fo that I ought to have added 1′2″ to the time of the clock, whereas I fubftracted as much from it. This is the reafon of the difference in the times. The fmall variation in the diftances of the limbs was owing to a ftricter verification of the parts of my micrometer.

I am, with the moft refpectful efteem,

Gentlemen,

Paris, Feb. 14, 1764.

your moft obedient,

humble fervant,

Pingré.